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**Some Legacies of
Robbins' Nature and
Significance of Economic
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**SOME LEGACIES OF ROBBINS'
NATURE AND SIGNIFICANCE OF ECONOMIC SCIENCE**

**Address To Conference Celebrating
the 75th Anniversary Of Lionel Robbins'**
Essay On The Nature And Significance Of Economic Science.
(to be published in a special edition of *Economica* honouring Lionel Robbins)

by

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ABSTRACT

This paper criticises three Robbinsian positions still often found in modern economics: (1) the methodology of intuitively obvious assumptions; (2) treating facts as illustrations rather than as tests of theoretical propositions; (3) assuming that theory provides universally applicable generalisations independent of the characteristics of individual economies and so are independent of specific historical processes. Two corollaries of point (3) are that theory cannot assist in explaining unique historical events such as the emergence of sustained growth in the West and that economists need not interest themselves in the details of the technologies that produce the nation's wealth.

Key words: methodology, economic generalisations, measurement, positive economics, historical specificity

JEL classification: B41, B31.

SOME LEGACIES OF ROBBINS'

NATURE AND SIGNIFICANCE OF ECONOMIC SCIENCE

Lionel Robbins was a great human being. He accepted and lived by liberal values. He was appalled by events in Europe, particularly Nazi Germany and he did his best to help refugees from that terror. Among other things, he did a great deal both emotionally and financially for a colleague of ours who had spent much of the war in a German concentration camp and was the only one of his family to escape the gas chamber.

He espoused the importance of a rational approach to problems and of the power of economics to assist in it. Asking himself about the value of his subject, he wrote (1935, p.152):

“Surely it consists in just this, that, when we are faced with a choice between ultimates, it enables us to choose with full awareness of the implications of what we are choosing. Faced with the problem of deciding between this and that, we are not entitled to look to Economics for the ultimate decision... But, to be completely rational, we must know what it is we prefer. We must be aware of the implications of the alternatives. For rationality in choice is nothing more and nothing less than choice with complete awareness of the alternatives rejected. And it is just here that Economics acquires its practical significance. It can make clear to us the implications of the different ends we may choose. It makes it possible for us to will with knowledge of what it is we are willing. It makes it possible for us to select a system of ends which are mutually consistent with each other.”

I. Personal Background

I first encountered Robbins' essay as an undergraduate in 1949. I was mightily impressed and learned much from it. But I balked when I came to his discussion of the place of facts in economics. I read: “The propositions of economic theory, like all scientific theory, are obviously deductions from a series of postulates. And the chief of these postulates are all assumptions involving in some way simple and *indisputable* facts of experience....(1935, 78 Italics added). If the premises relate to reality the deductions from them must have a similar point of reference” (1935, p.104).

I read and reread this material and said to myself: “This cannot be right; facts derived from empirical observation must be more important to the development of theory than to act as *ex post* illustrations of what we already know to be true.”

Some four years later I entered the LSE as a PhD student and attended Lionel Robbins' great Wednesday afternoon seminar. In an age of increasing specialisation, this seminar was a breath of fresh air. Everything in economics was grist for Robbins' mill. At the beginning of each year, Lionel would ask around to see what were thought to be the most important new ideas in the subject and then make them seminar topics. Sometimes we had a different topic each week and at other times, as with Patinkin's *Money Interest and Prices*, we spent a whole term on one publication. The sense of being Renaissance people interested in any and every topic in our subject was exhilarating.

As the weeks passed, however, Lionel's expressions of the then prevailing methodology described above revived my interest in his essay. As the theories we discussed in the Wednesday seminar became based on increasingly complex and less intuitively obvious assumptions, we found ourselves frustrated by the inconclusiveness of arguments concerning their intuitive plausibility. A group of us who were thinking along the same lines formed the LSE staff seminar on Methodology Measurement and Testing in Economics that became known as the M²T seminar, which Jim Thomas discusses in another paper in this volume. We talked to philosophers of science such as Joseph Agassi and, a bit later on, Imre Lakatos (who became a good friend of mine). Agassi introduced us to Popper and under his influence we came to reject the Robbinsian methodology and accept the position that economic theories were to be judged by the ability of their predictions to stand up to empirical testing. We disagreed with Friedman's (1953) argument that only predictions were to be tested against evidence and held that if a theory's predictions pass test in spite of being derived from assumed relations that were empirically false (such as all demand curves have positive slopes), we learn by asking why? (To discuss this matter fully requires distinguishing among the various uses of assumptions in economic theory. I have discussed the issue of empirical relevance of assumptions in some detail in Lipsey (2001).)

From 1960 to 1963, I wrote *An Introduction to Positive Economics* which was designed to promote the methodology of testing as opposed to the Robbinsian methodology of intuitively obvious assumptions. The book had an immediate impact and went through five reprints in the four-year life of its first edition.¹

While there is much that I could say in praise of Robbins' essay, given the space constraints, I must concentrate on my criticisms. Here I want to take up three issues that I think pose serious problems, all of which have modern manifestations: the methodology of intuitively obvious assumptions, the relegation of facts to be illustrations of theoretical propositions rather than as tests of their validity, and the belief in the general applicability of economic theory without the need for specific context. It would take someone better versed in the history of economic thought than I to determine where Robbins stood in the chain that leads from the first statement of each of these ideas to their modern manifestations. There can be little doubt, however, that Robbins was an important link in their transmission to modern economists, both where he was initiator and where he was such a superb populariser that he helped to make many of them the conventional wisdom of economics for generations to come.

II. The Methodology Of Intuitively Obvious Assumptions

At the outset of his essay, Robbins states his main thesis that the "generalisations" of economic theory are both certain and empirically relevant: "The efforts of economists over the last hundred and fifty years have resulted in the establishment of a body of generalizations whose substantial accuracy and importance are open to question only by the ignorant or the perverse" (1935, p.1). Lest we have any doubt about their relevance: "It is a characteristic of scientific generalisations that they refer to reality" (1935, p.104). Lest we have any doubt about their certainty: "[O]ur belief in these propositions is as complete as belief based upon any number of controlled experiments" (1935, p.75).

What is hard for modern economists looking back from today's vantage point to believe is that we really did spend the bulk of our time discussing the plausibility of the assumptions of the theories we were attempting to assess. Lest one thinks we were some local backwater, I can attest that as London secretary from 1954 to 1957 of the Oxford-Cambridge-London joint economics seminar where graduate students and junior staff in the three universities met to hear papers and exchange views, the Robbinsian methodology was dominant. So when we switched from the Robbinsian to the Popperian methodology, we made a sea change in how we approached our subject.

The view that economics can be about the world and yet be based on intuitively obvious assumptions pervaded much of economics long after Robbins wrote. For example, much economic theorising of both a positive and normative sort was, and still is, based on the twin assumptions that technology and tastes are given and that the latter can be expressed by a utility function in which the goods and services an individual consumes are the only arguments. The first is typically thought to be an assumption of mere convenience although it is not innocuous because when technology is assumed to be endogenous, as it undoubtedly is in the real world, many comparative static results are altered, some even reversed. (For elaboration see Lipsey et al (2005, Chapter 2) and Lipsey (2007a, p. 335).) The second assumption was long thought by many (most?) economists to be intuitively self-evident. Only in the last decade or so has it been called into question. Sen's (2000) capabilities approach was an elegant redirection away from this assumption and further research as summarised by Layard (2005) showed that it could be seriously challenged on empirical grounds. Hence, the strong advice given, then and now, about policy measures required to increase economic welfare were, and still are, based on a very shaky foundation. (I have discussed this in much more detail in Lipsey (2007a).)

Another assumption that I think most of us took as self evident, that "bygones are forever bygones," was discussed at length by Robbins (1935, p.52). This is an important normative proposition for those who wish to maximise something. But as Robbins would have it, a self-evident assumption on which to base positive predictions about behaviour, current research has shown it to be flawed. People's behaviour often reveals them to be acting as if bygones did matter. (For examples, see Tversky and Kahneman (1992) and Kahneman and Thaler (2006) and for another general critique of Robbins' a priorism see Blaug (1992, pp. 76-79).)

III. Facts As Illustrations Rather Than Tests

Having taken his position on the truth of assumptions and predictions, Robbins had to come to terms with the place of factual observations in economics and he outlined what he saw as their three main uses. First, as "...a check on the applicability to given situations of different types of theoretical constructions" (1935, p.116). For example, did something offsetting occur that violated the *ceteris paribus* assumption? Second, behaviour, and hence the applicability of a particular theory, may depend on second order assumption such as institutional constraints on what can be done. Thus the facts may suggest "auxiliary postulates" that are needed. For example, regulatory constraints on banking behaviour, require amendments to the predictions from the theory of unregulated bank behaviour. Third, factual observations may expose "...areas where pure theory

needs to be reformulated and extended. They bring to light new problems” (1935, p.118). The meaning of “extended” seems clear enough: issues that current theory has not investigated are revealed. But “reformulated” sounds suspiciously like using empirical studies to reveal the need to amend the theory. Unfortunately, the example Robbins gives covers *extension* to new areas but not the *reformulation* of existing theories, so we cannot be sure what he intended here. (I do not find the discussion of this point on page 118-120 altogether clear so it is possible that I have not quite grasped what Robbins intended, but I think I am approximately on track here.) In summary: “Realistic studies may suggest the problem to be solved. They may test the range of applicability of the answer when it is forthcoming. They may suggest assumptions for further theoretical elaboration. *But it is theory and theory alone which is capable of supplying the solution*” (1935, p.120 italics added).

Not only are facts not to be used to test theories, measurements of aggregates are typically irrelevant to theory. For example: “Estimates of the social income may have a quite definite meaning for monetary theory. But beyond this they have only *conventional* significance” (1935, p.57). Later, Robbins asks: “Ought we not to wish to be in a position to give numerical values to the scales of valuation, to establish quantitative laws of demand and supply?” (1935, p.107). He then goes on to argue that with demand elasticity “...*there is no reason to suppose that uniformities are to be discovered.*” Thus such measurements taken at a particular time and place have no “...*permanent* significance—save as Economic History” (1935, p.109 italics in original). I do not read this passage as does Sutton (2008) as a warning that specific measurements taken at one time and place cannot necessarily be generalised to other times and places. Instead, I take Robbins to mean that the truths of economics are all about the signs of changes, such that all demand curves have negative slopes, and not about magnitudes, such as measured values of the income and price elasticities of most foodstuffs fall as incomes rise going well below unity in high income countries. I criticised this position in the first edition of *An Introduction to Positive Economics* (1963, pp. 158-161) on three grounds. First, even if *a priori* reasoning suggests that a particular relation will not stay constant over time, only empirical observation can establish if this is so. Second, it is important to know just how stable or unstable any relation is. For example, if demand curves shifted in location and slope drastically and capriciously over short periods of time, none of the comparative statics of price theory and their policy applications would be of much use. Third, even if there are substantial variations in the relation under consideration, only empirical observations can show if these variations appear random, in which case we have done everything we can by way of explanation, or systematic, in which case the presence of an as-yet-unobserved causal variable is suggested.

In the early days of the M²T seminar, I think we were naïve enough to believe that one theory replaced another in the history of the subject when evidence that conflicted with the incumbent could be better explained by the challenger. Soon, however, we came to accept Imre Lakatos’s more subtle view of how scientific paradigms are related to evidence and how one replaces the other. (I have stated my current understanding of this issue in Lipsey (2000).) Although naïve falsification is open to serious criticism, and although cases where theories have been rejected due to tests of their predictions are not common in economics, two Popperian methodological messages seem to stand up and to be something that all students of economics should be taught. First, economic theories

that are consistent with all possible states of the world are empirically empty and, as such, they tell us nothing about the real world whose behaviour we seek to understand and predict. They may be very general devices that can be used as receptacles for further empirically based assumptions that specify lower-level theories that are not empirically empty but their usefulness depends on this being so. Second, even if direct tests of theories that do have empirical content are not all that frequent, such theories are *in principle testable* by looking for the observations that they rule out. (As Popper stressed, these are statements about the logic of scientific statements not necessarily about the process by which scientific advance occurs.)

In arguing this view in the article “Positive Economics” in the forthcoming revision of the New Palgrave Dictionary, I put it this way:

‘First, if an economic theory is to be about the real world, it must be possible to imagine observations that would conflict with it. If conflicting observations cannot even be imagined, the theory is compatible with all states of the world and hence empirically empty. A great advance in making theory more relevant would be achieved if today’s editors insisted that each author state what factual observations would conflict with his or her theory, and, if there were none, to state the theory’s purpose. Second, a new theory should be compatible with (‘explain’) some existing facts and suggest some new one(s).’

Instead, all too many articles follow the Robbinsian use of facts as mere illustrations of the applicability of theory. The ‘test’ then is: “Can the theory or model be made to track already known facts?” In doing so, the authors are applying what Popper criticised as “sunrise tests”: predicting what we already know and nothing else. From this we learn something about the ingenuity and technical skills of the authors but not much more.

The Robbinsian view on the relative unimportance of facts as controls on theorising persists in a surprising amount of modern economics. The approach gives rise to what I call “internally driven research programs” (IDRPs), programs that are driven by attempts to understand problems created by the programs’ models rather than problems arising from empirical observations related to the models. An IDRPs often begins with a factual question. Typically a simple model is developed yielding strong answers. Investigators then ask would these predictions stand up if we altered the model to make it more realistic. What, for example, if we went from one, to two, to three sectors? What if we made saving endogenous rather than exogenous? What if we let technology change in response to the market signals? — and so on and so on. If empirical observations are used in the program’s later stages, the question is usually of the sunrise variety: Can the model be made to track the data? Many of the fads and fashions that sweep economics are aspect of internally driven research programs. I am not arguing that internally driven programs never produce interesting results, only that the probability of their producing new empirically relevant results is quite low (even when the models are made to track already known data).

In Lipsey (2001) I gave a number of illustrations, the most dramatic being the burst of theorizing concerning economic growth that started in the 1940s and ran to about 1970. Harrod and Domar produced the then-famous knife edge result of a completely

unstable growth path. Concern over this disturbing result was allayed by Solow's famous neoclassical growth model in which the absence of a fixed capital/output ratio produced a stable, balanced growth path. Solow's work led in two directions. One was the empirically based growth accounting exercise that looked for the sources of the residual that Solow had attributed to technical change—and that turned out to be due to a much more complex set of influences than just technical change. The other was the theoretical investigation of growth models. This second direction produced an IDRPs as the profession embarked on a 15-year bout of balanced growth research. At the end of it all Amartya Sen (1970, p.33, italics added) had this to say:

"The policy issues related to economic growth are numerous and intricate. ... While the logical aspects involved in these exercises are much better understood now than they used to be, perhaps the weakest link in the chain is the set of empirical theories of growth that underlie the logical exercises. *Possible improvement of policies towards growth that could be achieved through a better understanding of the actual process of growth remains substantially unexplored.* It is partly a measure of the complexity of economic growth that the phenomenon of growth should remain, after three decades of intensive intellectual study, such an enigma. It is, however, also a reflection of our sense of values, particularly of the preoccupation with the brain-twisters. Part of the difficulty arises undoubtedly from the fact that the selection of topics for work in growth economics is guided much more by logical curiosity than by a taste for relevance. The character of the subject owes much to this fact."

Reviewing the same literature at a much later date I put the same point this way: "[I]nternally generated questions produce [only] internally directed answers." (Lipsey, 2001, p 181). (For another example, this time with respect to the effects of rent control, see Lind (2007) and for more general discussion of the issue see Goldfarb and Ratner (2008).)

What I call "externally driven research programs" (EDRPs) contrast sharply with IDRPs. These are programs that are driven by, and constrained by, observed facts. The growth accounting search to explain the Solow residual is one good example. Another is the evolution of theories concerning monetary and fiscal policy through the long set of debates between Keynesians and old fashioned monetarists that raged through the 1950s, '60s, and 70s. The conclusion can be dated at 1980 when the Keynesian, James Tobin, joined the monetarist, David Laidler, in a debate that was subsequently published in the *Economic Journal* (Tobin 1981 and Laidler 1981). Tobin and Laidler disagreed on some matters of judgement about speeds of reaction and the precise slopes of some curves. They revealed, however, no discernible differences of underlying models or of fundamental assessment of what were the key relations that governed the economy's behaviour and what were the key policy conclusions regarding fiscal and monetary policy. This 30 year debate generated much heat in its time, but the end result was much light. Empirical evidence about such things as the income and the interest elasticities of the demand for money, and wage and price flexibility, was amassed. The extreme position of each of the two schools was moderated in the light of the accumulating

evidence, until their differences were slight compared with their agreements.²

IV. Absence of Context Specificity

This is a pervasive characteristic of much modern economics. I discuss it in four parts, raising issues on which economists are still strongly divided.

Universally applicable generalisations

Robbins was certainly a major link in the chain of *How Economists Forgot History* (Hodgson 2002). Economists once actively discussed what Hodgson calls historical specificity, but which my co-authors and I prefer to call context-specificity: there is a trade off between the generality of a theory and its empirical content. Robbins clearly lies at one extreme in this issue. He argues that the generalisations of economics are universal, applying to all times and all places. For example (1935, p. 80): “It has sometimes been asserted that the generalisations of Economics are essentially “historico-relative” in character, that their validity is limited to certain historical conditions, and that outside these they have no relevance to the analysis of social phenomena. This view is a dangerous misapprehension”. The reason, of course, is that in Robbins’ view the main *assumptions* of economics are not historical relative.

This takes a very strong line on a debate that still rages among economists, both explicitly and implicitly. There are many illustrations of the non-Robbinsian side, of which the following are only a few examples.

- The preference functions that drive behaviour do appear to vary across societies, at least to some extent. For example, Michael Porter (1990) has noted the differences in risk-taking behaviour among business persons in societies such as Japan and Germany where business failure is regarded with strong disapproval, and societies, such as the US, where a failure or two on the road to final success is taken as acceptable, even normal. For some applications, therefore, the preference functions need to be specific to certain geographical and/or historical circumstances.
- The serious problems encountered in the rush to marketise the former USSR’s command economy stemmed partly from a view that economic theory of markets showed them working as long as all impediments were removed, irrespective of institutions and other factors that are not modelled in canonical neoclassical market theory, but which distinguish one economy from another. Appropriate policies for marketising a command economy, therefore, depend on the specific national context of such things as existing institutions, leaned behavioural modes, and levels of development.
- The IMF and World Bank’s one-size-fits-all view on correct economic policy for all times and places, as enshrined in the policy of ‘structural reform,’ has proven to be a failure, at least when it had gone beyond removing the most extreme non-market policies. This is now recognized even by those two institutions and the discredited view is being replaced by an understanding that different policies may be relevant for the different conditions facing various developing nations. (For examples see Griffiths (2003), Hira (2007), Rodrik (2006) and Stiglitz (2002).)

One of the reasons why many neoclassical economists hated second best theory³ is because it showed that since a “distortion-free economy” is an impossibility, all policy advice has to take place in second best situations where context specificity is all important. Because different societies face different “distortions,” making second best improvements depend on the specifics of each set of market conditions. For example, removing ‘distorting’ subsidies on a poor country’s production of a product heavily subsidised by the U.S. may eliminate the local industry, add to unemployment, and accentuate balance of payments problems. (I have dealt with these issues in more detail in Lipsey (2007a).)

No economic analysis of historical processes

At one point in his analysis Robbins asks (1935, p. 131) “...:can we not frame a complete theory of economic development?”. Having answered in the negative, he goes on to argue (1935, p. 133) “Nor are the prospects improved when we turn to the sphere of technical change and invention.... What technique of analysis could predict the trends of inventions leading on the one hand to the coming of the railway, on the other to the internal combustion engine?”.

Robbins’ negative answer is partly because he recognised comparative statics as the only valid tool of economic theorising and could not see this dealing with dynamic issues of economic growth and technical change. Of course, modern macro-growth models with either exogenous or endogenous technical change do theorise about economic development, even if they cannot deal with such specific questions as the coming of the railways or the invention of the internal combustion engine.

Even among modern growth theorists, however, a major difference in approach reveals a conflict between Robbinsian universal applicability and non-Robbinsian context specificity. Growth theories that use an aggregate production function, with either exogenous or endogenous technical change, are devoid of detailed specifications of technology – which usually appears as a scalar either pre-multiplying the aggregate production function or as a variable in that function – of institutions, or of anything else that distinguishes one economy from another. Thus, they yield the universal prediction that when certain basic conditions are fulfilled with respect to such things as saving and investment, growth will inevitably follow in all countries.

The few existing formal growth models that use a more structured representation of technology are either explicitly or implicitly context specific. For example, after presenting our three-sector model of sustained GPT-driven growth with endogenous technological change, we conclude (Lipsey et al 2005, p.467):

“Our models implicitly assume the institutional circumstances that underpin modern market economies, such as private property, limited liability, and the rule of law. They also assume the specific institutions involved in the West’s invention of how to invent...[that] made the West’s growth process self-sustaining.... Because of their structure, they apply only to countries whose growth depends to a significant extent on developing from their own resources new technologies, both fundamental and derivative. Thus, they are not meant to apply to countries whose growth processes are more or less completely driven by

the diffusion of technologies developed elsewhere. Nor are they meant to apply to those whose GPTs are currently static and who seek conditions that would allow them to begin a period of sustained growth.

All these qualifications illustrate once again the issue of historical specificity: the richer the explanatory power of a theory and the more predictions that it makes, the more restricted is its range of applicability in both time and space. Finally, we observe that there is no single ‘correct’ way to make the historical specificity trade-off. All growth processes have things in common, and to deal with these, very general theories are helpful. But all growth processes also have many aspects that are more specific in time and place. To deal with these, and, therefore, to get to deeper levels of explanation requires less generality and more specificity.”

Explaining the emergence of sustained growth

The conflict between Robbinsian generality and historical specificity can also be seen in a big way in theories of how the West turned the episodic growth of earlier eras into the sustained growth that was initiated by the two Industrial Revolutions. The highly popular unified growth theories (UGTs) that were introduced some time ago by Galor and Weil (2000) are fully in the Robbinsian tradition. They model an economy developing from a long period of extensive growth in which all increases in output are taken up by increases in population, through a transition period, and then into a modern period of intensive growth in which the rate of population growth falls well below the rate of output growth. UGT models are general, containing nothing that would distinguish one economy from another. As a result, they predict that any country could have endogenously generated its own industrial revolution and the resulting transition to sustained growth; all that was needed was a sufficient passage of time.

In contrast, most economic historians argue that local conditions were important in generating the West’s Industrial Revolution and the sustained growth that it ushered in. There is debate about the proximate causes but most of these are to be found in things that distinguished the West from the rest — i.e., they are context specific.⁴ My co-authors and I emphasise Western science as an obvious necessary condition for the Second Industrial Revolution in the later part of the 19th century — a body of knowledge that was absent everywhere outside of the West. Somewhat more controversially, we argue that Western science in the form of Newtonian mechanics, was a necessary condition for the First Industrial Revolution — a body of knowledge that was also absent outside of Europe and best established in Britain.

No interest in technological details

Robbins forcefully argued what is still a commonly accepted view about the irrelevance of any detailed knowledge of technologies for understanding the growth process. “The technique of cotton manufacture, as such, is no part of the subject-matter of Economics...” (1935, p.33). “Economists are not interested in technique as such” (1935, p.37-8). “The precise shape of the early steam engine and the physical principles upon which it rested are no concern of the economic historian as economic historian—although

economic historians in the past have sometimes displayed a quite inordinate interest in such matters” (1935, p.41).

This is still a common view so that most students and many theorists of economic growth do not see any need to have the kind of knowledge found, for example, in Usher’s *History of Mechanical Inventions* (which is the book I suspect Robbins had in mind in labelling such interest “inordinate”). The opposing view is that technical change lies at the heart of long term economic growth and that to understand such growth sufficiently to develop policies to influence its magnitude and direction requires a detailed knowledge of technologies and of how they change such as is found in the writings of Nathan Rosenberg and Alfred Chandler Jr. (See for example Rosenberg (1982 and 1994) and Chandler (1977 and 2001).)

Here are just two examples of this non-Robbinsian view. We argue in Bekar and Lipsey (2004) and Lipsey et al (2005) that the transition to sustained growth brought about by the two Industrial Revolutions was to a great extent the result of the culmination of three trajectories of technological advance that combined scientific and technological developments over several centuries. The first was the steam engine whose modern trajectory began in the 16th century with investigations into the nature of steam and of vacuums and culminated with the development of the high pressure engine at the beginning of the 19th. The second was automated textile machinery whose research program was charted and begun by Leonardo di Vinci late in the 15th century and culminated when the centuries-long trajectory of inventions and improvements produced machines that it paid to transfer from cottages to proto-factories in latter part of the 18th century. The third was electricity whose modern development began with the publication of Gilbert’s *De Magnete* in 1600. This put the West decisively ahead of China in understanding magnetism and electricity by making it a science rather than a piecemeal collection of individual observations. (For full discussion see Pumfrey (2002).) The trajectory evolved through countless discoveries and applications and culminated with the invention of the dynamo in 1867, which ushered in the electronic age in which we are still living. We describe these three critical trajectories in detail in Lipsey et al (2005), pages 243-4 for mechanized textile machinery, 249-52 for the steam engine and 254-5 for electricity. To understand them fully, why, how, and when, they occurred, why they did not occur outside of the West, and why they turned episodic into sustained growth, one needs to know a lot about technologies, including much of what is in Usher’s great book.

For a second example, Vernon Ruttan (2006) has argued that recent changes in US institutions make it increasingly difficult for the US to develop new general purpose technologies (GPTs), which are the main engine of long term growth. One cannot assess the strength of his argument, or its policy implications if it is true, unless one knows a lot about the technical details of GPTs – not just the kind of abstract models that are found in Helpman (1998), useful through they are; one needs to know the engineering details of GPTs and how they evolve over decades to become highly efficient, and universally used for multiple purposes. (For a discussion of Ruttan’s argument see Lipsey 2007b.)

V. CONCLUSION

Although most modern economists would reject Robbins belief that economic theory can develop propositions that are simultaneously certain and empirically relevant,

many accept, either explicitly or implicitly, other of Robbins' beliefs, some of which I have criticised in this paper. Many assumptions, such as maximising behaviour, are treated as self-evident propositions on which theory can be safely erected. Facts are often used to illustrate rather than test theories, as when tracking known facts is the only test that a new theory or model is asked to pass. Some theoretical propositions are taken as universally correct in the absence of any context-specificity, such as that removing a 'market distortion' will inevitably increase efficiency and/or economic welfare. Theories of growth devoid of any characteristics that would distinguish one economy from another are often thought to be satisfactory and generally applicable, as for example is the case with unified growth theories (UGTs) and those models of growth that are based on a neoclassical aggregate production function either with exogenous or endogenous technical change. Debates about the causes of, and policies to affect, long term growth driven by technical change are often thought capable of being dealt with satisfactorily without any knowledge of the details of either the nature of technologies or the processes by which they change.

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End Notes

¹ The successor to this book, now called *Economics*, by Lipsey and Chrystal is currently in its 11th U.K. edition. The American adaptation was first published in 1968 as *Economics* by Lipsey and Steiner and is now in its 13th U.S. edition authored by Lipsey, Ragan and Storer. How much credit the first few editions of *Positive Economics* should get for replacing the Robinson methodology with a more empirically oriented one in the U.K. is for others to judge. But within a decade or so after its publication the old methodology had disappeared so completely that students found it hard to believe that serious economists assessed theories by arguing about the intuitive plausibility of their assumptions. As the proportion of the population attending university rose slowly but steadily over the years, the sophistication of first year text books was steadily reduced. As a result, the long anti-Robbinsian first chapter on methodology, and the chapters labeled “Criticisms and Tests” that ended each section were progressively simplified and finally eliminated.

² Just as that apparently satisfactory situation was being reached, a challenge arose in the form of the more basic critique of the Keynesian-monetarist synthesis, made by the new classical economists led by Robert Lucas. Although this new classical macro economics dominates modern advanced macro textbooks, Keynesian income-flow models still dominate the analysis of practical policy problems by central banks and departments of finance and these utilize much of the knowledge amassed during this long Keynesian Monetarist debate. I have discussed and assessed these developments in more detail in Lipsey (2000, p.69-80).

³ As Lionel said to me over lunch shortly after our original second best article had been published: “But my dear Dick it is *so* nihilistic!”

⁴ One exception is Kenneth Pomeranz (2000) who argues that there was at least one other country, China, that could have endogenously generated its own industrial revolution and sustained growth. Our reasons for dissenting from this view are spelled out in detail in Lipsey et al (2005, Chapter 8).